MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXII. *May* 10, 1872. No. 7.

PROFESSOR CAYLEY, President, in the Chair.

Report on Observations made by the Government of India on the Total Eclipse of the Sun on Dec. 11–12, 1871. By Lieut.-Col. Tennant, R.E., F.R.S. (Abstract.)

This paper is an official report on the observations of the Total Eclipse of December last, which was observed by Lieut.-Col. Tennant and his coadjutors by the orders of the Indian Government.

A letter to Dr. Huggins, published at page 70 of the present volume of the *Monthly Notices*, will have given the Fellows of the Society some knowledge of what was observed, and as Col. Tennant's paper will appear at greater length it will not be necessary to make this notice very long.

Dodabetta, the station of observation, is the principal peak of the Nilgherry Hills, situated in N. Lat. 11° 24′, E. Long. 76° 43′. Its height above the sea is 8650 feet nearly, and the observations made here, and especially the photographs (of which glass transparencies were exhibited at the meeting), are valuable for comparison with those of the observers of the British expedition whose stations were at a far lower level.

Col. Tennant expresses himself very positively as to the absence of any well-defined and nearly uniformly bright portion of the corona whose extent is limited to some four or five minutes from the Sun; up to the limit to which it was examined the light

faded away. He also is confident that those portions of the corona to which his attention was directed were constant, and this is evidenced not only by his own photographs but by comparison with the pictures taken by Lord Lindsay's assistant, Mr. Davis, at Bekul, as there exists the most marked resemblance.

The spectroscopic results are described as in the paper before mentioned. Identifications of the lines are given, but of course these are liable to some considerable uncertainty. The prominent point is that the green line considered to be identical with K 1474 was apparently continuous across the rifts. Of course there was no possibility of recording the numerous lines which Capt. Herschel saw as the solar limb appeared. It is the fact that they were so numerous which is of importance.

As regards the photography the report gives some details as to the procedure, and then proceeds to describe the photographs. These descriptions would hardly be intelligible without the pictures themselves, copies from which were examined with much interest by many of the Fellows. The corona is extremely beautifully depicted in the four early photographs, while the fifth, though less sharply defined, is still exceedingly good. The sixth is defective, evidently from the clock having ceased to act; and as it was stated that it was going after the set was completed, it seems not improbable that one of the clamps which had slipped once before had again temporarily yielded. The encroachment of the corona and prominences on the lunar limb is very marked, and it produces the *elongated appearance* of the Moon which Colonel Tennant, in his letter to Dr. Huggins, mentions having noticed in the hurried examination he was able to give before it left. The transparencies on opal glass were much admired, and do great credit to Captain Waterhouse, who made them from the negatives, which are themselves the result of his manipulation.

As regards Polariscope observations, Col. Tennant gives a result by Mr. Broughton, B. Sc. London, who found no polarization before total phase, but that Savart's Polariscope showed the characteristic bands brightly at the instant when the coronal light appeared.

Colonel Tennant concludes by giving the results at which he has arrived from an examination of the results reported in this and his preceding paper, and a comparison with those known to him as having been attained by others. He says:—

"The following, then, seems to be the constitution of our Sun. There is a nucleus which gives out continuous white light like solid or liquid bodies, and even dense gases. Surrounding this is a layer of heavy vapours intensely heated, but far less so than the nucleus, and in which, if a state of equilibrium could exist, the heaviest vapours would be lowest. Above this is a layer of glowing hydrogen of very slight density accompanied by that gas which gives out the line D_3. Still further up these gases in a cooler state become mixed with what gives out the green line K 1474; and, lastly, that alone seems to remain."

Colonel Tennant has deposited the original negatives of the photographs taken on this occasion with the Astronomer Royal.

On Errors in Vlacq's (often called Briggs' or Neper's) Table of Ten-figure Logarithms of Numbers. By J. W. L. Glaisher, B.A., F.R.A.S., Fellow of Trinity College, Cambridge.

As, with the exception of Vega's *Thesaurus &c.*, the only complete table of ten-figure logarithms that has been published is the original one, partially calculated by Briggs and completed by Vlacq, the publication of errors detected in this work is of very considerable importance. Lists of errata have been given by Vlacq himself, by Vega, Sherwin, Lefort, and others, but it is not possible to find without comparison to what extent these lists are concurrent or supplementary; the main object of this note is, therefore, to give the results of an examination of these tables of errata, and to supplement them with some not previously published. In order to render clear what follows, it is necessary to premise some facts with regard to the original calculation of the logarithms of numbers.

The first table of logarithms to the base 10 was calculated by Henry Briggs, Savilian Professor of Geometry at Oxford, and published by him under the title, *Arithmetica logarithmica, sive logarithmorum chiliades triginta, pro numeris naturali serie crescentibus ab unitate ad* 20,000*: et a* 90,000 *ad* 100,000. . . . *Londini, excudebat Gulielmus Jones,* 1624.* This table contains the logarithms of the natural numbers from unity to 20,000, and from 90,000 to 100,000 to 14 places of decimals. There is thus left a gap from 20,000 to 90,000, which was filled up by Adrian Vlacq, who published at Gouda, in 1628, a table containing the logarithms of the numbers from unity to 100,000 to 10 places of decimals. Having calculated 70,000 logarithms and copied only 30,000, Vlacq would have been quite entitled to have called his a new work. He designates it, however, only a second edition of Briggs, the title running, *Arithmetica logarithmica, sive logarithmorum chiliades centum, pro numeris naturali serie crescentibus ab Unitate ad* 100,000. . . . . *Editio secunda aucta per Adrianum Vlacq, Goudanum.* . . . . *Goudæ excudebat Petrus Rammasenius,* 1628. This table of Vlacq's was published, with an English explanation prefixed, in London in 1631. The title of the English work is, *Logarithmicall Arithmetike;*

* Some copies, with the same title-page, contain an additional chiliad; viz., the logarithm of numbers from 100,000 to 101,000. The library of Trinity College, Cambridge, contains one of these copies, which belonged to Dr. Brinkley. In 1617 Briggs published his *Logarithmorum chilias prima.*

or, Tables of Logarithmes for absolute numbers, from an unite to 100,000. . . . . *London, printed for George Miller*, 1631.

Speaking of Briggs' *Arithmetica Logarithmica* of 1624, De Morgan, in his article on Tables in the English Cyclopædia, says, "After his [Briggs'] death, in 1631, a reprint was, it is said, made by one George Miller; the Latin title and explanatory parts were replaced by English ones—'Logarithmicall Arithmetike,' &c. We much doubt the reprint of the tables, and think that they were Briggs' own tables, with an English explanation prefixed in place of the Latin one. Wilson (in his *History of Navigation*, prefixed to the third edition of Robertson) says, that some copies of Vlacq of 1628 were purchased by our booksellers, and published at London with an English explanation premised, dated 1631. Mr. Babbage (to whose large and rare collection of tables we were much indebted in the original article) has one of these copies; and the English explanation and title is the same as that which was in the same year attached to the asserted reprint of Briggs. We have no doubt that Briggs and Vlacq were served exactly in the same manner." On referring to Robertson (Fourth Edition, p. xvi.), I find no further information than that contained in the above extract. That the above suggestion of De Morgan's is correct, and that Miller's and Vlacq's tables are both printed from the same types, is evident on examination. Miller's copies are not quite so rare as they might be inferred to be from the above remarks; there is one in the University Library, Cambridge, and one in the library of Trinity College, Cambridge; while the library of the Royal Observatory at Greenwich contains two; there is, however, no copy in the libraries of the Royal Society or the Royal Astronomical Society. On the fly-leaf of the first of these copies (viz. that in the University Library) there is written in pencil, "N.B. These tables were edited by Vlacq and printed at Gouda." Three out of the four copies mentioned (the Trinity one is the exception) contain, after the English introduction and before the tables, a title-page, on which is printed, *Tafel der Logarithmi voor de Ghetallen van* 1 *af tot* 100,000, while the corresponding page in Vlacq's table is inscribed, *Chiliades centum Logarithmorum pro numerus* (sic) *ab unitate ad* 100,000. This shows the Dutch origin of Miller's table; and it is besides curious, as it seems to imply that Vlacq meditated a Dutch translation of his work, but that the tables intended for the purpose were bought and published by George Miller. A careful examination of any one page of Vlacq's or Miller's copies is sufficient to show that they were printed from the same types; a good confirmation is afforded by the following error: in the numbers 3376, 3377 . . . . 3380 were printed instead of 4776, 4777 . . . . 4780; to remedy this a small piece of paper, on which the latter numbers were printed, was stuck over the former, and this occurs in both Vlacq's and Miller's copies. This and similar facts place beyond all doubt the identity of the tables, whether published in Gouda or London, and to

establish this fact is important, as we are thus assured that the same errata-list suffices for both. It is this table (which in Miller's copies, from the occurrence of both names on the title-page, is generally called after Briggs or Neper, and in the original copies after Vlacq) which was referred to at the beginning of this note.

In 1631 Vlacq published his *Trigonometria Artificialis*. This work contains, among other tables, the logarithms of the numbers from unity to 20,000, printed also (with the exception of the last sheet referred to further on) from the same types.

No further calculation of logarithms of numbers took place till the end of the last century, when the great French manuscript tables (the *Tables du Cadastre*) were computed under the direction of Prony. These, as is well known, have never been published.

The lists of errata which will be particularly referred to are,—that prefixed to Vlacq's own table in the *Arithmetica Logarithmica*,* that given by Vlacq on the last page of the table of the logarithms of numbers in the *Trigonometria Artificialis*, that given by Sherwin, and the elaborate table of M. Lefort, in t. IV. of the *Annales de l'Observatoire de Paris*, pp. [148]–[150], which was obtained by comparison with the *Tables du Cadastre*. This list also contains the errors given by Vega in the *Thesaurus logarithmorum completus*. It is intended to be supplementary to Vlacq's list in the *Arithmetica*, so that the two taken conjointly should give all the errors. One of the copies in the Greenwich Observatory library belonged to Maskelyne, and the following note is written on the cover: "All the errata have been corrected that had been discovered by, and are contained in William Gardiner's book (the author of the logarithm sines, &c., to every 10 seconds). Others, 22 in number, marked T, have been corrected, which were discovered by Michael Taylor, author of the logarithm sines and tangents to every second of the quadrant. Nevil Maskelyne, Astron. Regius, March 23, 1790."

I do not know where the errata found by Gardiner and Taylor were published, if at all: Gardiner's are not given in his tables. It is very probable that Taylor's were communicated personally to Maskelyne; it is worth notice that though the above note is dated 1790, yet Taylor's logarithm tables were not published till 1792. I have made no special search for either of the lists referred to, as the correction of the errors in the volume was sufficient for the purpose I had in view; viz., the discovery of errors not included in the lists of Vlacq and Lefort. The result of a careful comparison between Maskelyne's copy and Vlacq's and Lefort's list of errata was the detection of the following seventeen errors, not given by Vlacq in the *Arithmetica*, nor by Lefort.

* This list does not occur in Miller's copies.

Number.	Error.	Correction.
1360	89083	89084
1622 Diff.	26,76609	26,76699
2154	56970	56990
3192	13,62358	13,60358
7117 Diff.	6,10169	6,10179
9329 Diff.	4,75506	4,65506
9610 Diff.	4,51869	4,51896
*11275	65506	65505
11293	93280	93281
11699	87409	87410
18723	54375	54373
18724	86323	86325
23999	4,34019	4,38019
24580	18785	18786
53250 Diff.	81555	81557
*54040	53404	53403
64818	56295	56265

In the case of the two numbers marked with an asterisk the error is of no importance whatever, and not worth correcting; thus, the logarithm of 11275 is 4·05211, 65505, 49998, 14 . . ., and it is a matter of indifference whether the tenth figure of the mantissa be increased or not; log. 54040 = 4·73271, 53403, 49992, 98 . . ., and the same remark applies. These two logarithms were easily calculated by means of the logarithms up to 1200 to 20 places given by Callet, since $11275 = 25 \times 451$ and $54040 = 280 \times 193$.

On subtracting the logarithm of 14485 from that of 14486, the difference is 299814 instead of 299813, and Maskelyne's copy is accordingly so corrected; the book is, however, correct, as on referring to Briggs' table of 1624 the true difference is 2998132 . . ., the unit difference being due to the correction of the last figures of the logarithms in Vlacq. All the errata in the above list I have examined carefully, and satisfied myself that they are accurate. The errors corresponding to the numbers 1622 and 7117 also occur in Briggs' table of 1624.

The following are errata in Lefort's errata-table. Corresponding to the number 63747 the number in the error column should be 974 instead of 947. For the number 53053, the correction there given is 3 instead of 2 in the last figure, but the logarithm ends with a 1 and not a 3, and on calculating it from Callet ($53053 = 77 \times 689$) I find that it is correct in the book. The correction probably has reference to some other number. For the number 50996 the correction 4 is printed so faintly as to be almost illegible, and for 16399 and 16699 the letter N is omitted.

Vlacq's errata-list in the *Arithmetica* contains no error; the

correction belonging to the number 14694 is superfluous in all the copies I have seen, the difference being printed correctly.

The following facts may be of interest: Vlacq's list in the *Arithmetica* contains 120 errata (106 of which correspond to numbers below 20,000). These are all corrected in Maskelyne's copy.* Lefort's list contains 452 errata, 301 of which are marked with an asterisk, to imply that they were given by Vega. Out of the 452, however, 287 may be called unimportant, denoting by this word errors which only alter by unity the last figure of the logarithm. There are 265 errors given by Lefort, not corrected in Maskelyne's copy, of these 222 were unimportant, and only 43 serious. There were, however, three corrections in Maskelyne's copy that were inaccurate, the original number being correct. The list in the *Trigonometria* (which only gives the logarithms of numbers from unity to 20,000) contains 79 errata in the logarithms and 40 in the differences; of the former three are not given in the *Arithmetica* (viz., those for the numbers 6197, 9182, 9429); they are to be found in Lefort. Of the errors in the differences 15 are new, while 5 (viz., those for the numbers 11400, 11499, 12582, 13639, 19081) which are given in the *Arithmetica* are omitted. Why these were not included I cannot guess; they are all important and uncorrected in the *Trigonometria.* The sheet (signature M) giving the numbers beyond 19500 is reprinted for the *Trigonometria*, and the error in 19960 (number) is corrected.

At the end of the preface to Sherwin's Tables† is a list of errata, those not given by Vlacq being marked with an "a." The error for the number 2167 should not be so marked, as it is to be found in both the *Arithmetica* and *Trigonometria;* three (viz., those for the numbers 6197, 9182, 9429) are in the *Trigonometria* and not in the *Arithmetica;* they therefore appear also in Lefort. The corrections belonging to the numbers 56359 and 57756, though marked with an "a," are not to be found in Lefort; they are, however, superfluous, as the logarithms are printed correctly in the copies I have seen: it is probable that they referred to indistinct figures in the copy Sherwin made use of. On the whole, Sherwin added twenty-one errata, of which one was not new and two were superfluous; the remaining eighteen are to be found in Lefort. Sherwin's list only gives errata in logarithms, not in differences. I have not made an examination of Vega's errata-list, feeling no doubt that this was completely done by Lefort.

Having found an error in one of the numbers, which was not

* In one instance the correction was wrongly made.

† Of Sherwin's Tables, De Morgan remarks in the article previously cited. "Second edition, 1717; third revised by Gardiner, and the best, 1742; fifth and last, 1771, very erroneous—the most inaccurate table Hatton ever met with." My father's copy, the errata-list in which I used in the above comparison, is dated 1726, and contains a list of "errata for the second edition of Sherwin's Mathematical Tables," by Gardiner.

given in any of the above errata-lists, it occurred to me that errors in the numbers would have stood a far better chance of escaping detection than errors in the logarithms, as the latter would be examined more or less by all succeeding editors of logarithmic tables; I have therefore made an examination of the numbers from 1 to 100,000 to discover misprints. The numbers from unity to 21,500 I examined myself, the rest were done for me by an assistant. The result is the discovery of errors in the following numbers:—

969	*16699
2081	19960
2082	34163
2260	49831
2278	*66150
3500	*66789
7368	*66790
12301	*69489
12345	*69490
16327	77052
*16399	

It is unnecessary to give the errors in the above numbers; the correction being, of course, evident at sight. The corrections for the seven numbers to which the asterisk is prefixed are contained in Lefort. Of the above twenty-one errors, twelve are corrected in Maskelyne's copy. It is probable that Vlacq's list in the *Arithmetica*, Lefort's, and the two lists in this paper, taken conjointly, contain nearly, if not quite, all the errors in Vlacq's tables. The total number is thus 603, of which about one-half are unimportant; the tables contain over 2,100,000 figures liable to error, so that there is one error, on the average, to every 3500 figures, or an important error to every 7000. Considering that the tables were the result of an original calculation, and that many of the errors were caused by the slipping of figures after the proofs had left the author's hands, the great accuracy of the table must be admitted by all. Two trivial errors may be noted; viz., Chilias 28 should be Chilias 38 in the first column of the page beginning with the number 37051, and the number in the corner of the page beginning with the number 66001 should be 66151 instead of 66251. In the course of the examination of the numbers, 49 were marked as containing figures so imperfectly printed, that they might lead to error; on referring to another copy, this was the case with but 25 of these; viz., 576, 4576, 7106, 16826, 19650, 21286, 24077, 30420, 31176, 31226, 33326, 37088, 41426, 45876, 61226, 61526, 66876, 66896, 81026, 83864, 96903, 97318, 97326, 97328, and 98280. Of course, in many copies these may be printed quite legibly, but the occurrence of imperfections in them in two copies renders it probable that they

may be similarly imperfect in others. It might also be advisable in any copy to ascertain that the slip of paper, previously noticed, containing the numbers 4776–4780, had not come off and been lost; but in point of fact the only part of the table ever used is that for the numbers 10,000 to 100,000, for which the characteristic is 4. It may be remarked that several of the alterations in Maskelyne's copy are made in the text, and these at first sight might give the impression that all the copies were not printed from the same types; such, however, is not the case.

The increase of the last figure in tables, when the succeeding figures are greater than 500 . . ., seems to deserve more attention than it has received. Errata, such as some noticed in this communication, where the succeeding figures are 499 . . ., are by no means uncommon; and it appears that the discoverers of them imagine they are doing some service by noting them. Take, for example, one of the cases in this note: the figures starting from the tenth are 5 49998 . . .; if we take 5 as the tenth figure, the error is 49998 . . ., if 6, the error is 50002, differing by 00004. Now, as our table only professes to give 10 places correctly (regard being paid to the magnitude of the figure in the eleventh place), a difference in the fifteenth place does not come in question at all: 5 and 6 are both equally correct; they only differ by quantities, which throughout all the rest of the table we agree to neglect. It is a matter of regret that all such valueless refinements are not avoided by the author always explaining the exact convention on which the last figure is increased. A very convenient arrangement would be to understand that when x figures of a number were tabulated, the error was less than 6 in the next figure; or, if the calculator wished to be more accurate, 5 6 in the next two figures. To obtain a table of x figures, it is usual to calculate $x + 1$, or $x + 2$ figures, and the inconvenience of extending the calculation further in the particular case when the next figure is 5, or the next two 50, is, in many cases, excessive, and as the result is of no additional value when obtained, a figure "wrong" under these circumstances ought not to be styled an error. Probably a good many of Lefort's errata are of this class. Babbage, in the introduction to his well-known table of seven-figure logarithms, states, that in ninety-three instances the next three figures in Vega were 500, and that in all these cases the logarithms were carried to more than ten places to determine whether the figures were really 500 . . . or 499 . . ., and decide whether the least figure was to be increased or not. This appears to me to have been quite needless. It sets up an unnecessary and artificial standard of accuracy for the numbers whose seventh, eighth, and ninth figures happen to be 4,9,9 or 5,0,0. To the user of a table of seven-figure logarithms it is a matter of really no importance whether his error is 499 or 501; he is content to make an error of 5, and an additional error of $\pm$ 001 is of no consequence.

It is not a little remarkable that the most accessible table of

ten-figure logarithms we possess should have been published nearly 250 years ago. This places their use out of the power of all who have not access to an important library. In mathematical computations, ten-figure logarithms are very often wanted, even when only 5 or 6, or even a less number of figures of the ultimate function are intended to be tabulated. Most functions are expansible in an ascending (convergent) series of the form $A_0 + A_1 x + A_2 x^2 + \ldots$, and a descending (semi-convergent) series of the form $\frac{B_1}{x} - \frac{B_2}{x^2} + \frac{B_3}{x^3} - \ldots$. The former is usually very convenient for values of x less than unity, and moderately so till $x = 3$ or 4 perhaps, while the latter may, from the convergent portion, give the required number of places from, say, $x = 12$ or 20; but between the two practicable ranges of the two series there is usually a gap, and it is in cases such as these for the calculation of the intermediate values (if there is no continued fraction available for the purpose) that ten-figure logarithms are of the greatest use.

The title of Vega's work is *Thesaurus Logarithmorum completus ex Arithmeticâ logarithmicâ et Trigonometriâ artificiali Adrian Vlacq . . . a Georgio Vega. Leipsiæ*, 1794 (folio). The whole work contains 684 pp. of tables, of which the logarithms of numbers from unity to 100,100 occupy 305 pp. The arrangement is similar to that generally in use for seven-figure tables, and a great saving of room is effected: this advantage, however, is neutralised in the present work for the computer who only requires the logarithms of numbers by the other tables that are added. It is to be inferred from the preface that the publication of the tables was due to the great scarcity and expense of Vlacq's works; but, at all events in England, Vega's work is more scarce now than the latter. Many abridgments of Vega have been published.

Mention must be made of a work published last year; viz. *Tables de Logarithmes vulgaires à dix decimales, construites d'après un nouveau mode par S. Pineto . . . S. Petersbourg*, 1871, which by a new and ingenious arrangement in effect gives a complete table of ten-figure logarithms in 56 octavo pages. It does not seem quite so convenient as the older tables, and I imagine any one who can obtain the latter will prefer to use them.

Mr. Sang, of Edinburgh, has announced the intended publication of a nine-figure table of logarithms of numbers from unity to a million, which will be of great value to mathematical calculators; but, even with this work, the additional figure in Vlacq will prevent his table from being superseded entirely.

Trinity College, Cambridge.

Note on the Discovery of Saturn's Second Satellite.
By Richard A. Proctor, B.A.

In the obituary notice of Sir John Herschel which appeared in the *Daily News* for Saturday, May 13, 1871, the following passages occur. "It was long supposed that by the use of the great 40-feet telescope, and in the first moment, Sir. W. Herschel had discovered the sixth satellite of *Saturn*" (the second according to our present system of nomenclature); "and Sir John Herschel himself believed it till a controversy in 1843 occasioned a reference to a private journal of his father's, by which it appeared that it was the 20-feet telescope which was the instrument of the discovery." "We have referred to a controversy about the 40-feet telescope in 1843. At the meeting of the British Association at Cork that year, Dr. Robinson paid his tribute of admiration to Lord Rosse's telescope, and in the course of his remarks, said that however honourable the great Herschel telescope was to the philosopher and to the king of that day, it must be regarded as a failure, if judged by what it had effected. Sir John Herschel contested this assertion, fell into the great mistake of attributing to this instrument a discovery made with another, acknowledged his error, but continued his plea, &c."

Without attributing to this matter (really quite insignificant) the importance attached to it by the author of the above-mentioned obituary, I yet consider that as the point has been raised it is desirable that it should be rightly settled. I venture therefore to call attention to some evidence which seems to show that the belief accepted before 1843, was after all correct. It may be that those who remember the controversy then raised can correct me; but so far as the evidence cited in the obituary notice is concerned, I thing the rebutting evidence I shall bring forward is sufficiently convincing.

It appears that reference was made to a private journal of Sir W. Herschel's. Now I would venture to point out that a private journal would be much more likely to contain a faulty record than a public announcement. For an announcement communicated to a learned society would be carefully worded even as first written, and it would be revised (perhaps more than once) before being passed for press. Whereas we all know that a record may be hastily and carelessly written in a private journal. So that comparing directly the evidence of the private journal with Herschel's account at page 10, part I., of the *Phil. Trans.*, for 1790, I should be disposed to attach much more weight to the latter. It will be known to all who read this that in the account here referred to Herschel distinctly ascribes the discovery of *Enceladus* to his 40-feet telescope. But as this announcement was made some time after the discovery there may be some who will regard it as less likely to be correct than a record made at the time in a private journal. There is confirmatory evidence, however, which so far as I am aware has not been noticed in any

of our astronomical treatises. At the close of a catalogue of 1000 new nebulæ and clusters of stars, in the *Philosophical Transactions* for 1789, Part II., page 255, a postscript of seven lines occurs, which runs as follows: "The planet *Saturn* has a sixth satellite revolving round it in about thirty-two hours, forty-eight minutes. Its orbit lies exactly in the plane of the ring, and within that of the first satellite. An account of its discovery with the 40-feet reflector, and a more accurate determination of its revolution and distance from the planet will be presented to the Royal Society at their next meeting."

The paper to which this postscript is added was read June 11, 1789; but the satellite was not discovered until August 28, 1789. Doubtless the postscript was inserted by Herschel within a few days of the discovery of the satellite. It is at any rate the first published notice of the discovery; and its agreement with the full account published in the *Philosophical Transactions* for 1790, Part I., is very significant.

To this may be added the circumstance, that Struve, to whom Sir John Herschel presented his father's complete series of papers, with manuscript annotations by Sir W. Herschel, goes out of his way, so to speak (since he is writing about the stars) to mention that "le grand télescope de 40 pieds, achevé en 1789, conduisit immédiatement à la découverte du 6me satellite de *Saturne*." *Etudes d'Astronomie Stellaire*, note 59.

When this paper was read I mentioned that it was my wish to elicit further information on the subject. Mr. Lassell thereupon pointed out that there had been a discussion in the columns of the *Athenæum* in which Mr. Robinson had, as he believed, gone far to establish his case. Mr. Lassell also mentioned that Sir W. Herschel had used his 40-feet telescope so far back as 1787, and had seen the second satellite with the 20-feet telescope in the same year.

Dr. Huggins and Mr. Lassell have since very kindly supplied me with further notes from the *Philosophical Transactions*.* These notes, combined with those I have supplied above, seem to give a complete explanation of the whole matter. They may be arranged as follows:—

First, as to the construction of the 40-feet telescope, Herschel says in the *Phil. Trans.* for 1795 (p. 350), "I had my first view through it on Feb. 19, 1787; but I do not date completing the instrument until later. . . . . I continued to work upon it till August 27, 1789. . . . . On August 28, 1789, having brought the telescope to the parallel of *Saturn* I discovered a sixth satellite of that planet."

Secondly, as to the observation of the satellite in 1787, Herschel says, in vol. lxxx. of the *Phil. Trans.* (his paper is dated

* I should mention that I have not convenient access at present to other volumes than those containing Herschel's researches into the fixed stars.

November, 1789), "I should certainly have been able to announce its existence as early as August 19, 1787, when at $22^h\ 18^m\ 56^s$, I saw and marked it down as probably a sixth satellite, then about 12° past its greatest preceding elongation. . . . In the year 1788 very little could be done towards a discovery as my 20-feet speculum was so tarnished by zenith sweeps that I could hardly see the *Georgian* satellites. In hopes of great success with my 40-feet speculum I deferred the attack on *Saturn* till that should be finished, and having taken an early opportunity of directing it to *Saturn*, the very first moment I saw the planet, which was the 28th of last August, I was presented with a view of six of its satellites in such situations, and so bright as rendered it impossible to mistake or not to see them; and also on the 17th Sept. I detected the seventh satellite when at its greatest preceding elongation." In ascertaining the period of the sixth satellite, Herschel states that he used the 19th August, 1787, as a starting-point. Later he states that the seventh satellite "appears in the 40-feet, no bigger than a very small lucid point," yet he says, "I see it very well with the 20-feet reflector, to which the exquisite figure of the speculum not a little contributes." This is the account referred to in the former part of this paper.

It seems demonstrated then that though the satellite was seen in 1787 (as Hind also mentions), with the 20-feet telescope, it was not discovered in any proper sense of the word until August 28th, 1789, the instrument of the discovery being the 40-feet telescope. If the observation of 1787 is to be regarded as the discovery of the satellite, then by parity of reasoning Herschel did not discover *Uranus* nor did Galle observationally discover *Neptune*.

Note on the Densities of Jupiter's Satellites.

By Richard A. Proctor, B.A.

Incorrect values of the densities of *Jupiter's* satellites have somehow found their way into our text-books of astronomy, and have been repeated from one to another. They have led to erroneous assumptions respecting the condition of these bodies.

Thus, in Lardner's *Handbook,* we find the following table: —

Satellite.	Mass, that of Jupiter = 1.	Mass, that of Earth = 1.	Density, that of Earth = 1.	Density, that of Water = 1.
I	0·0000173	0·00520	0·02016	0·1143
II	0·0000232	0·00698	0·03015	0·1710
III	0·0000885	0·02663	0·06984	0·3960
IV	0·0000427	0·01285	0·03925	0·2225

It is strange that, though accepting different values of the satellites' diameters, Mr. Chambers, in his *Descriptive Astronomy*, gives the same values for the densities, only omitting the last decimal figure.

Whence these values were originally derived I cannot say. They are unquestionably incorrect, and are, in fact, not even near the true values. This is easily shown in any given case. Thus take the second satellite, whose diameter is a little over 2000 miles, or about $\frac{1}{40}$th part of *Jupiter's* mean diameter. Then, if of equal density with *Jupiter*, its mass would be about $\frac{1}{64000}$th part of *Jupiter's*, or would be represented by 0·0000156. But Laplace's estimate of the mass of this satellite is 0·0000232, or more than half as great again as that resulting from a density only equalling *Jupiter's*. Hence the satellite's density is more than half as great again as *Jupiter's*. But *Jupiter's* density is represented by 0·24 if the Earth's is taken as unity; and by 1·36 if the density of water is taken as unity. Hence this satellite's density would be represented by more than 0·36, if Earth's equal 1, and by more than 2·04 if the density of water = 1.

So much to show that the values tabulated by Lardner and Chambers are erroneous whencesoever obtained.

The following values of the densities of the several satellites have been obtained by combining Laplace's estimates of the mass, with the values of the diameters given in the second column:—

Satellite.	Diameter in Miles.	Density, Earth as 1.	Density, Water as 1.
I	2352	0·198	1·148
II	2099	0·374	2·167
III	3436	0·325	1·883
IV	2926	0·253	1·468

It will be observed that all the satellites except the first thus appear to have a greater mean density than *Jupiter*. Probably their real densities are greater than those here tabulated, since irradiation would increase their apparent diameters.

Note on the Orbit of the Double Star Castor.

By J. M. Wilson, Esq.

The orbit of *Castor* appears to be hyperbolic, a form of orbit which, as far as I am aware, has not been shown to exist in the case of any binary system.* Mr. Gledhill was good enough to furnish me with a list of measures of *Castor*, extending from A.D. 1740 to the present time, and on charting these on a table of

* [It may be interesting to compare with Mr. Wilson's hyperbolic orbit the elliptic orbit deduced by Mr. Hind from all the observations with which he was acquainted, ranging over the period from 1718 to 1845. In the *Monthly Notices* for December 1845, Mr. Hind remarks that "the elements are entirely different from those previously computed by Sir John Herschel and M. Mädler; and this difference is materially owing to the great influence exerted by recent measures at Mr. Bishop's observatory, by Mr. Dawes." The results "are as follows" (we quote from a note by Mr. Dawes in vol. xxxv. of our

engraved squares, and drawing and correcting the interpolating curve according to Herschel's method, I obtain the following eleven points for the determination of the orbit:—

θ.	*t.*	*r.*
340	1740·46	11450
330	52·44	10588
320	63·14	10188
310	73·34	10065
300	83·49	10119
290	93·89	10320
280	1804·89	10700
270	16·89	11256
260	30·39	12054
250	46·45	13254
240	66·49	15343

When the points are laid down they lie nearly on a hyperbola of eccentricity 2·2. The real hyperbola may be also shown by a graphical construction to have an eccentricity = 3·16, and its

Memoirs), "those given by Capt. Jacob at about the same date being added for comparison:—

	Mr. Hind in 1845.	Capt. Jacob in 1846.
Perihelion Passage	1699·26	1703·30
Projected place of Perihelion	° 8 15	° 10 0
Node	11 24	10 0
Angle between Perihelion and Node on Orbit	356 22	0 0
Inclination	43 14	43 17
Eccentricity	0·2405	0·300
Mean Annual Period	− 34′·163	− 33′·072
Semi-axis Major	6″·300	6″·30
Period	632·27 yrs.	653·1 yrs.

Capt. Jacob considered his results as only a rough approximation. Remembering that Sir John Herschel had obtained the period 253 years, while Smyth had obtained 240 years, we recognise the influence of recent observations, or rather of the progress of the motion of the component stars, in pointing to an increase of the orbit's extension. Mr. Wilson's result involves a somewhat startling advance in the same direction.

It may be added that the observations of Bradley and Pond give for the position-angle towards the close of the year 1720, 355° 53′.

The following measures are given by Dembowski in a recent number of the *Astronomische Nachrichten*:—

	Position-angle.	Distance.
	°	″
1866·02	241·07	5·384
1870·237	240·2	5·30
·327	239·5	5·64
·349	239·6	5·41
·68	239·34	5·488
1871·185	238·1	5·50
·324	239·3	5·59

It is worthy of notice that the study of this fine double star first impressed on Sir W. Herschel's mind "a full conviction," to use his son's words, "of the reality of his long-cherished views on the subject of the binary stars."—Ed.

line of nodes nearly coincides with the axis major. I hope to find time in the summer to examine the orbit by analytical methods.

The arc at present described is not sufficient to allow of a reliable graphical solution.

If my orbit is correct, the angle of position will decrease towards the limit of 188°. It is now about 238°·12, according to the observations of Mr. Seabroke and myself, and a little less than this by the interpolating curve, about 237·85. Perhaps some observers would communicate their measures during the past four or five years.

It would seem that *Castor* is a difficult star to measure. Some of the observations taken about the year 1865 especially must be in error by at least 3°, even on the mean of several nights.

On the Nutoscope, an Apparatus for showing graphically the Phenomena of the Earth's Motion called Precession and Nutation. By Prof. Ch. V. Zenger.

(*Communicated by John Browning, Esq.*)

In the case of a rapidly-revolving solid, as the Earth's, round its axis, two things may take place, according as the mass of the solid body is or is not uniformly distributed round the axis of rotation. In the first case the axiswill steadily hold its position during the rotation, as is seen by rotating the top of a gyroscope. But if the motion of the rotating solid body is disturbed by a shock or overweight acting on one side, the axis of rotation will describe a cone and its apex a circle. This effect is produced by the attraction of the Sun and Moon on the Earth, conceived as a sphere rotating round its axis. The equatorial plane changes then periodically its points of intersection with the plane of the ecliptic, which we call the precession of the nodes.

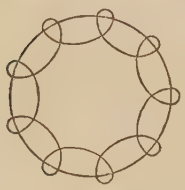
In the second case, if the mass of the gyroscope is not equally distributed round the axis, and even this unequal distribution becomes changeable, which is practically done by putting on the axis of the rotating gyroscope a circular plate, with an excentric hole fitting loosely to the axis, the motion becomes more complicated, and the apex of the axis gets a double motion, a circular by the overweight acting as disturbing force on one side, and an elliptical produced by the change of position of that overweight, for the friction of the hole on the axis produces a retardation of the velocity of rotation of the circular plate. There will be described, as fig. 1 shows, a circle with a small ellipse revolving on its periphery, the centre of the ellipse being placed on it. The angle of the cone described thus will be changing periodically, and to a small extension, exactly as the complicated phenomenon of the earth's precession and nutation is due to the changing attraction by the Sun and Moon at

Fig. 1.

different distances and positions on the flattened or spheroidical earth-ball. The apex of the gyroscope in that condition represents, therefore, exactly the same kind of motion as that produced by the cumulated action of Sun and Moon on the rotating Earth; and this apparatus may, therefore, serve to show in a very plain and instructive manner the complicated astronomical phenomena of precession and nutation.

This is done by the apparatus that I call a Nutoscope. A very carefully-worked top of the known form is placed on a brass stand with a small spherical hole on its upper part, in which the sharply pointed axis of the rotating top is placed. The other end of its axis bears also a sharp point, that is slightly pressed on a blackened paper held by the hand in a quite horizontal position. It requires some practice to get a good drawing of the precessional and nutational curves by hand, and so I disposed the apparatus in such a way as to allow, by lowering with a micrometrical screw, a plate of paper fastened tightly on a brass ring moving along a cylindrical stand of brass near and perpendicularly to the axis of the top in its upright position. Another ring is fastened, with its plane parallel to the plane of the top in its upright position, the intersection of both planes giving the position of the nodes, the circular disk of the top representing the Earth's equator, and the plane of the ring the plane of the ecliptic. Giving the axis of the top by a fastened overweight an inclined position, the cone is described and the precession of the nodes shown on the periphery of the ring, and the apex of the top's axis describes on the blackened paper a circle.

Putting an additional changeable overweight, viz., the circular disk with the excentric hole on the top, the apex describes beautiful curved lines produced by the compound precessional and nutational motion, as shown in the adjoining diagram. The greater the weight of the circular disk, and the slower the rotation becomes, the larger become the elliptic motion and the more distended are the curved lines. The same effect is produced by the friction of the apex of the top axis, which can be conceived as a changeable disturbing force: it is, therefore, difficult to get a top that would not give those pretty curves of nutation of its axis.



A great variety of designs of these curves may be got by changing the form and weight of the disk, the ellipses may be got so narrow that they become only visible by aid of a microscope. The appearance of these curves may become in some degree a means to account for the equal distribution of the mass in such apparatus as gyroscopes, tops, &c.

If the velocity of rotation is rapidly diminishing by the resistance as friction, &c., the top's axis describes with a fastened overweight a spiral instead of a circle, and with a changeable overweight there is described a spiral line with an ellipse revolving with its centre on the spiral line.

Note on ξ Ursæ Majoris. By J. M. Wilson, Esq.

The subjoined measures of this double star were taken by Mr. Seabroke and myself:—

t.	*θ.*	No. of Readings.	*r.*	No. of Readings.	Observers.
	°		″		
1871·44	43·9	(4)	..	..	W
1872·13	23·15	(7)	1·04	1	W & S
·18	22·95	(14)	·97	5	W & S
·20	23·3	(4)	·91	2	W & S
·21	23·0	(6)	1·19	(2)	W
·24	20·3	(3)	..	..	W
·35	19·7	(4)	1·18	2	W

It is near its periastron, as I pointed out in the *Astronomical Register* of February, and its angular motion is very rapid. I do not know what weight to attach to this series of observations as they do not agree very well with those given by Mr. Knott, a very much more experienced observer. His measures given in the *Monthly Notices* for March are:—

t.	*θ.*	*r.*
1872·028	31·6	1·092
·088	29·69	1·111
·138	28·15	1 052

It deserves careful watching at present. Its period is now shown to be between 61·0 and 61·2 of a year.

Note on ζ Cancri. By J. M. Wilson, Esq.

This very interesting binary of short period has recently passed through its periastron; and I annex my observations of it, which may be valuable, as it was not observed at all at its last periastron in the beginning of the century:—

t.	*θ.*	*r.*
1872·18	168·6	0″·78
·19	169·3	0″·63
·25	165·25	..

All the measures were taken with Alvan Clark's $8\frac{1}{4}$-inch refractor and a parallel wire-micrometer.

Mr. Christie kindly observed the star at my request, and allows me to communicate his measures made with the Greenwich Equatoreal:—

t.	*θ*	*r.*
1872·33	157·6	0″·4±.

Stars very tremulous.

Temple Observatory, Rugby, May 9, 1872.

Elements of Minor Planet (119).* By M. Péchule.

From observations made at Ann Arbor on April 3, at Paris on April 11, at Lund on April 19, and at Hamburg on April 28, I have calculated the following elements of this planet :—

Epoch 1872, April 28·5, Berlin M.T.

$$M = 164^\circ\ 21'\ 24''$$
$$\pi = 36\ 34\ 33$$
$$\Omega = 203\ 39\ 38$$
$$i = 6\ 30\ 30$$
$$\varphi = 8\ 3\ 11$$
$$\mu = 862''\cdot08$$
$$\log a = 0\cdot40964$$

May 2, 1872.

Observations of Planet (120)†. By Dr. C. H. F. Peters.

On the 11th inst. I found a small planet 11·5 magnitude, of which I take the pleasure to communicate the following positions, all that it has been possible to obtain during the unfavourable weather :—

1872.	H.C.M.T.	α (120)	δ (120)	
	h m s	h m s	° ′ ″	
April 11	14	12 0 5	−4 59 0	Approximate.
16	13 37 29	11 56 52·42	−4 44 39·1	12 comp. } with Schjel-
17	11 7 16	11 56 20·96	−4 42 15·8	10 ,, } lerup 4348.
19	11 52 8	11 55 13·69	−4 37 1·6	10 ,, with Lalande 22646.

Litchfield Observatory, Hamilton College, Clinton, New York, 22 April, 1872.

Eclipse of Jupiter's Third Satellite observed on April 11th, 1872, at Forest Lodge, Maresfield. By Capt. W. Noble.

The satellite which, as usual, faded out very gradually did not fairly disappear until $10^h\ 2^m\ 24^s$ L.S.T. = $8^h\ 40^m\ 56^s\cdot4$ L.M.T. The predicted Greenwich mean time of disappearance, in the *Nautical Almanac*, was $8^h\ 38^m\ 19^s\cdot0$, so that I *ought* to have lost sight of it at $8^h\ 38^m\ 36^s\cdot5$ L.M.T. (I assume my longitude to be 17°·5 E. of Greenwich), or $2^m\ 19^s\cdot9$ sooner than I did. This discrepancy, however, is much smaller than usual for one of the outer satellites. I employed a power of 154, adjusted on the planet, on my 4·2 inch Ross Equatoreal.

* [This planet was re-discovered by M. Paul Henry, at Paris, on April 9. The actual discovery must be credited to Prof. Watson, of Ann Arbor. Ed.]

† This planet is the same as that discovered by M. Borelly, at Marseiles, on April 10.

Occultations of Stars by the Moon, observed April 18, 1872, *at Maresfield.* By Capt. W. Noble.

Occultation of B.A.C. 3579 *by the Moon.*

The star disappeared instantaneously at the Moon's dark limb

At $9^h\ 48^m\ 37^s{\cdot}2$ L.S.T. = $7^h\ 59^m\ 40^s{\cdot}8$ L.M.T.,

and reappeared at the bright limb

At $11^h\ 6^m\ 19^s{\cdot}5 \pm 1^s$ L.S.T. = $9^h\ 17^m\ 10^s{\cdot}4$ L.M.T.

Afterwards I observed the

Occultation of i Leonis.

The star disappeared instantaneously at the dark limb

At $12^h\ 2^m\ 46^s{\cdot}0$ L.S.T. = $10^h\ 13^m\ 27^s{\cdot}6$ L.M.T.

Massive cumuli covered the Moon at the time of reappearance.

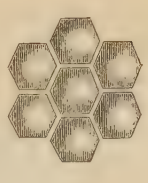
Power in each case 255, adjusted on the first-named star.

On the Value of the Stereoscope as applied to the Examination of Eclipse Photographs. By A. Cowper Ranyard, Esq.

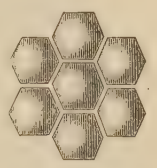
At the last meeting of the Society it was suggested, that by combining in the stereoscope two photographic pictures of the corona, the one taken at the beginning of totality and the other towards the end, good and reliable evidence may be obtained that the corona is a solar appendage. It was further asserted, that in such a combination, if properly made, the dark body of the Moon and the corona both seem to stand out as solid bodies. If such stereoscopic appearances may be relied upon, their value in proving the solar nature of the corona, as well as in showing the way in which the streamers are arranged upon the solar surface cannot be too much exaggerated; for without them we are in possession of the mere projections upon a plane of an arrangement of details which curve and interlace with one another in three dimensions.

It becomes, therefore, of the greatest importance for us to examine whether, by such a combination, it is possible to obtain any true stereoscopic effect — by which I mean an effect which is produced by the parallax of nearer objects upon those further from the observer.

It is well known that suitable shading will influence our minds in that compound process often called the judgment of the eye; and the sloping together of lines in apparent perspective is continually used by artists whose business it is to make space of two dimensions appear as if it was space of three.



The way in which we may be deceived by suitable shading is very well illustrated by the annexed drawing, which is made from a photograph taken a few years ago by Mr. Beck. The mere alteration of illumination caused by turning the drawing round will make the parts of the picture which in the one instance appear to be convex to appear as if they were concave.



It is also certain that our judgment is much less acute and discriminating when we view a picture through a lens than when it is looked at directly with the naked eye; for this purpose, single lenses are used to give apparent relief to photographic groups of flowers and other objects.

We cannot, therefore, be too careful in examining the real causes of any mental impressions that we may receive when looking at photographs through the medium of lenses, before we proceed to rely upon them as affording good proof of horizontal parallax between the two superposed pictures.

Let us now, therefore, examine what chance we have of finding such a difference caused by parallax in two corona photographs, taken, the one at the beginning and the other towards the end of totality. If there were four minutes of time between the pictures—which is, indeed, a longer period than has elapsed between any of the corona photographs that have yet been taken—the Moon in that time would have moved through about two minutes of arc, covering up on the one side and discovering on the other the solar prominences, as was clearly shown in 1860; and so far as the prominences are concerned we have a true effect of parallax, and reliable proof that the prominences are behind the Moon.

But when we come to the parts of the corona in which there is no covering and uncovering, our changes consist in the difference of distance from the Moon's limb of any details of the corona which may be detected upon a careful examination of the two photographs. By details I mean such clearly recognisable points or curve lines as exist *within* the corona—the outer boundary or contour of the coronal light depending evidently upon the amount of exposure or the different development of the two photographs.

Any such increase of distance from the Moon's limb on the one side, accompanied by a decrease of distance on the other side, may be regarded as sufficient proof that the shifting details are unconnected with the Moon, or, at all events, are not carried along with it in its motion; though it still remains for us to show from other considerations whether such details lie in front of, about, or behind the Moon. But as regards the nebulous and hazy portions of the corona we can have no such measurable and reliable proof, and we are left only to the natural inference of

our minds that the nebulous haze is probably connected with the details which it surrounds.

It is, again, the inference of the mind which, upon looking into the stereoscope, associates those details of the corona which are not covered and uncovered by the Moon with those details which are so covered up and uncovered, or rather with the solar prominences, for there is very little detail of the corona proper to be seen at points so near to the Moon's limb.

If two similar photographs of the corona, copied from the same negative, be placed side by side in the stereoscope, the same effect of the Moon standing out in front of the corona is not produced.

We may therefore regard the stereoscope as a rough and ready means of detecting horizontal parallax between two photographs, though a much more satisfactory determination of the existence and amount of such parallax may be arrived at by a careful measurement of the photographs themselves.

Secondly, as to what may be learnt with regard to the disposition of the rays of the corona from any appearances of relief that may be detected with the stereoscope.

During four minutes of totality, our Earth would move through an angle, with respect to the Sun, of somewhat less than ten seconds of arc; while the Sun would turn upon his axis (carrying, we may conclude, all the detail of his coronal rays with him) through an angle of about two minutes of arc. This would leave a total alteration in the position that the arrangement of coronal rays would present to us of about one minute fifty seconds of arc, an angle too small to give appreciable relief in the stereoscope. That such an angle is far too small may be seen from the consideration that the maximum possible shift given by such a revolution to the end of a ray extending to the distance of the Sun's radius from the photosphere would not subtend an angle of one second, as viewed from the Earth, and the shift of such rays as are visible to us would be much less.

We must therefore reject all such appearances of solidity and relief amongst the rays of the corona as spurious effects produced by shading or some of the other unknown causes which are apt to deceive the mind.

On the other hand, the stereoscope is undoubtedly useful as affording a ready means of superposing, and thus showing the absolute identity as to form of the details of the corona at the beginning and end of totality.

The Tables of Jupiter's Satellites. By J. Maguire, Esq.

The proposal of the Astronomer Royal, published in the January Number of the *Monthly Notices*, viz., that one observatory should be permanently devoted to the observation of the phenomena of *Jupiter's* satellites, has undoubtedly a pre-

ferential claim to the consideration of the Royal Astronomical Society, and I think I may safely add that it has an interest for many outside the Society, who are in the habit of using a telescope. As one standing near the threshold of the Temple of Science, but not, he fears, having in his possession the password which confers the privilege of entering and mingling with the votaries within, I am glad to find that the time has at last come when the judgment and influence of the Society are likely to be successfully employed in establishing such an observatory.

Of the want of a public observatory other than meridional, there cannot I think be a question. Col. Strange proclaims the want and points to the quarter from which support should come. It may be observed that whenever a new Ordnance map of these islands is declared upon authority to be wanted, theodolites and other instruments are very soon forthcoming for the work. And who will venture to say that England has spent so much money on theodolites and Armstrong guns that she cannot afford to set up a few equatoreals for the purpose of observing the satellites of *Jupiter* in order to construct new tables which would give to several pages of the *Nautical Almanac* a value which they do not now possess? Any idea of this kind is instantly put to flight by remembering the various liberal grants recently made by her Majesty's Treasury for the observation of the phenomena of solar eclipses, and the satisfactory provision made for the observation of the transit of *Venus* in 1874.

It is not pleasant then, to observe the rather desponding tone of this passage which I quote from the paper of the Astronomer Royal: "I see no prospect of obtaining a sufficient number of observations unless some observer would consent to give his entire observing energies to these observations."

Upon whom may I ask has fallen the mantle of the observer of *Gamma Virginis?*

Norwich, May, 1872.

On Astronomical Units. By C. C. De Crespigny.

In treatises on astronomical subjects I have observed that in English works, diameters and distances are expressed in British statute miles of 1760 yards each, in French works in kilometers, and I presume that in German and other works they are expressed in terms of the standard measure of length of the country in whose language the treatise may be written: also that in England we acknowledge another mile of 2026 yards, the sixtieth part of a degree of the equator, which, as it is also acknowledged in other countries, may be considered to have a greater right to be accounted a mile than the statute mile; at least when speaking on astronomical subjects. I have also read the remark in treatises above alluded to sometimes appended to a statement of the distance of a star

from the Sun or of a planet from the Sun, that the human mind fails to form any appreciation of such an enormous distance. As it is one of the pleasing duties of any particular science to eliminate doubts and want of appreciation from the minds of students, and as it is difficult for students of one country to form a just appreciation of weights and measures when expressed in terms used in countries not their own, and as it would be generally desirable to have uniformity in expressions of astronomical distances as there already is in those of masses and densities, I venture to make the following suggestion in the hope it may be considered worthy of thought by some member of your Society. It is simply this, that ingenuous youth should first be taught the value of the 60th part of a degree and call that one mile. Then call the diameter of the Earth 6875 miles instead of 8000, as at present. From that point let his mind be led to contemplate the size of the Sun, the diameter of which he may be taught to express either in miles or in Earth's diameters. Then, instead of telling him that the Earth is 92 or 93 millions of miles (British) from the Sun, and at the same time stultifying him by saying that the mind fails, &c.; let him simply be taught that the Earth is *only* 100 of the Sun's diameters from that body, and his mind will at once appreciate the fact that the Earth is in reality very near the Sun, and he will be able to grasp with ease the distances of the exterior planets expressed in the same terms. He might for his own amusement reduce these distances to British miles, and it is to be hoped that the interests of the British inch would not be endangered by this method.

In the same manner distances of fixed stars, wherever determined, might be expressed in terms of the diameter or radius of the Earth's orbit.

Sarawak, February 1872.

The Astronomer Royal has received from Mr. Stone the following communication, dated Royal Observatory, Cape of Good Hope, 1872, April 18:—

"I have printed now 1856, 1857, and 1858, and have 1859 ready for press, and 1860 well in hands; but I hope also to bring out the results for 1871 this year."

A Paper by Mr Brett, "On an Altazimuth Mounting for Telescopes, especially adapted for the use of Observers who have no permanent Observatory," with an illustration, will appear in the next number of the *Monthly Notices*.